THE

CONNEXION OF LIFE

WITH

RESPIRATION;

OR,

AN EXPERIMENTAL INQUIRY

INTO

THE EFFECTS OF SUBMERSION, STRANGULATION,

AND

SEVERAL KINDS OF NOXIOUS AIRS,

ON

LIVING ANIMALS:

WITH

AN ACCOUNT OF THE NATURE OF THE DISEASE THEY PRODUCE
ITS DISTINCTION FROM DEATH ITSELF; AND THE
MOST EFFECTUAL MEANS OF CURE.

BY EDMUND GOODWYN, M. D.

Arteria animam accipit è pulmonibus. Cicero.

PHILADELPHIA:
SOLD BY B. AND T. KITE, NO. 20, NORTH THIRD-STREET.
1808.

Sanguineum laticem, turgens quem vena reportat,
Atque in pulmonum subito penetralia vibrat,
Imbiturum illic, jam quidquid ab aëre sumpto
Ætheris expressum est:................................
....................Motu, proh! quantum est artis in illo!
Vivimus hac fabrica tantum, cessante perimus:
Machina nam nostra hæc non est hydraulica solum,
Pneumatica est etiam; auxilio spirabilis auræ
Indiget......................

POLIGNAC, *Anti-Lucretius*.

CONTENTS.

SECTION IV.

SECTION V.

SECTION VI.

SECTION VII.

INTRODUCTION.

THE outlines of this Essay were published, about two years since, in the Latin language; and several of the most eminent professional men honoured them with particular attention. A reception so flattering from intelligent criticism gave me an unexpected importance, and encouraged me to continue my labours with the hope of presenting them to the public in a more perfect form.

In the course of these labours many difficulties have arisen, and much exertion has been employed; and success has often favoured me: some questions however are still left undetermined, and some facts unexplained; but about these I am less anxious, as they are not immediately connected with the principal object of this inquiry: and, although some small obscurities may still remain, several facts are ascertained, which will, I trust, be of considerable use both in philosophy and physic.

The respiration of animals has long been a difficult problem in physiology, and no satisfactory solution could be given of it, whilst chymistry continued in an uncultivated state: the general facts were indeed attended to, and

conjectures proposed to explain them (*a*); but as long as conjecture *only* was used, doubt and uncertainty remained.

Within these few years the knowledge of chymistry has been much improved, and experiments have been applied to respiration with considerable effect; new facts have been discovered, extraordinary phænomena explained, and considerable obscurities removed; but some difficulties still remained to tempt the hopes of future labourers. In the course of this inquiry I have carefully examined the facts described by others, and instituted a variety of new experiments, which seem to have ascertained a principal use of respiration, its connexion with the motion of the heart and the other functions of the body.

The life of animals has also been a subject of attention at different times: for until we are acquainted with some of its essential qualities, we cannot expect to distinguish it from death. The ancient physicians seem to have been aware of this; but they did not labour to supply the defect.

About the middle of this century two writers of distinguished character entered upon the inquiry (*b*), and pursued it with great ardour and enthusiasm for a considerable length of time: in this pursuit they displayed much learning and ingenuity, and collected many valuable facts from the writings of others; but still their conclusions were unsatisfactory and their opinions are now fallen into neglect. This indeed will not be wondered at by such as

(*a*) Sir Isaac Newton imagined that the atmospheric air might communicate an acid vapour to the blood in the lungs, which was necessary to keep up the action of the heart. Optics, p. 355.

(*b*) Whytt and Haller.

have examined their works; for it is generally observed of them, that they often blend hypothesis with fact, and sometimes grasp at more than the mind can attain.

After this time the study of life was generally neglected: however, some professional men attempted to search for the distinction between life and death in the sensible qualities of the body. Observations were collected, and several marks proposed to the world to determine this distinction (*a*): but subsequent observation has shown them to be insufficient; and, after much research and altercation, it is now generally allowed, that we have hitherto no other means to determine on absolute death, but the presence of putrefaction (*b*).

To avoid the sources of these failures, I have endeavoured to consider the subject in a different manner (*c*), taking the living body when all the accidental signs of life are removed, applying to it those external powers which really do restore them, then attending to the place and circumstances of their first operation, and the immediate effect they produce: thus I was led to the essential quality of life, and consequently to the means of distinguishing it from death itself.

This distinction will be the more valuable, as it applies to every similar state of the body, whatever may have been

(*a*) Lettres sur la Certitude des Signes de la Mort, par Mons. Louis. Dissertatio an Mortis incerta signa minus à chirurgicis quam ab aliis experimentis. Winslow.

(*b*) Dissertation sur l'Incertitude des Signes de la Mort, par Mons. Bruhier.

(*c*) Dissertatio Inauguralis de Vita Corporis humani. Auctore J. T. Vander Kemp.

the original disease; and if it be properly attended to, we may always determine with certainty, whether a person be really alive or dead; and the bodies of our friends may now be consigned to the grave, before they become noxious to others.

But the benefits of this inquiry have extended still further.

From the result of the whole, I have been enabled to determine the nature of the disease that takes place from drowning, hanging, and breathing several noxious airs; to give it a descriptive name, and its proper place in the systems of nosology; and to point out the most effectual means of cure. Many remedies are recommended for this purpose in the directions of public societies of different countries, and all of them are said to have been employed with success; and since the assertion is favoured by some of the most respectable authors, both ancient and modern, no one can deny it without incurring the charge of presumption: but if it should appear here, that animals may be recovered from the disease with equal ease, without the concurring operation of a great many of them, a regard to truth, as well as public utility, ought to determine our choice. Besides, when it is remembered that a prepossession in favour of these remedies may occasion a considerable loss of time, and that a loss of time in this disease is often a loss of life, the interest of humanity should rise above general prejudice, and respect for authority give way to deliberate investigation.

In the whole of this inquiry, I have endeavoured to adhere closely to the method of *Analysis*, beginning with

effects, and ascending to their particular causes, and from particular causes to more general ones, with the assistance of *Analogy* and *Induction*, until I could proceed no further. Then I assumed the last conclusion as a general principle to explain the production of all the effects in a descending series to the first phænomena, where the analysis began; in this manner adding the *Synthesis*, and thus giving to the result all the evidence which the nature of the subject would admit.

To avoid the danger of being misled by the colouring of fancy or inaccurate observation, the experiments were repeated several times with great care and attention, and always in the presence of some judicious friends: but as the testimony of the senses is not always sufficient, and the most careful inquirer may sometimes err, I neither expect, nor even wish, the conclusions should be adopted, until the experiments shall have been repeated by others; and if it be afterward proved, that I have been mistaken in any part of them, I shall readily retract the assertion, and acknowledge the error: for the interest of truth and the welfare of mankind are of more value than the reputation of an individual; and, as M. Bonnet properly says, "*Un* J'AI TORT *vaut mieux que cent répliques ingénieuses.*"

LONDON,
May 6, 1788.

SECTION I.

TO ASCERTAIN THE GENERAL EFFECTS OF SUBMERSION ON LIVING ANIMALS.

IN the course of this inquiry, we propose to ascertain the general effects of submersion on living animals, and to trace their connexion with the action of the water on the body.

The first of these objects may be obtained by attending to the different changes that take place in living animals while they are immersed in water, and examining the appearances of the internal parts when the body is taken out; and the second, by observing the manner in which the water is applied to the body in submersion, and tracing its particular operation in producing these changes.

For the first purpose, I procured a large transparent glass bell, that would allow me to distinguish accurately the circumstances that took place within it; and when it was inverted, and filled with water, I put into it, at different times, several cats, dogs, rabbits, and other smaller animals, and confined them there till they had the appearance of being dead. As soon as they were put under the bell, I attended to the changes that took place in the body; and when they had lost the external signs of life, I opened the head, the breast, and belly, and examined the internal parts.

Whilst I was engaged in this business, the bodies of three unfortunate persons, who had been drowned, were brought to me, at different times, with permission to inspect the internal parts: these I examined with great care and attention, and compared the appearances with what I had seen in the other animals.

In these different experiments, I observed some variations in the external morbid symptoms, and in the appearance of the internal parts; but the order and succession of the symptoms, and the condition of the organs immediately connected with life, agreed uniformly with the following description.

When an animal is immersed in water, his pulse becomes weak and frequent; he feels an anxiety about his breast, and struggles to relieve it: in these struggles he rises towards the surface of the water, and throws out a quantity of air from his lungs. After this, his anxiety increases; his pulse becomes weaker; the struggles are renewed with more violence; he rises towards the surface again, throws out more air from his lungs, and makes several efforts to inspire; and in some of these efforts a quantity of water commonly passes into his mouth. His skin then becomes blue, particularly about the face and lips; his pulse gradually ceases; the sphincters are relaxed; he falls down without sensation, and without motion.

If the body be immediately opened, it has these appearances:

1. The external surface of the brain is of a darker colour than usual; but the vessels are not turgid with

blood, nor are there any marks of extravasation about them.

2. The cavity of the lungs contains a considerable quantity of frothy fluid; and the pulmonary arteries and veins are filled with black blood throughout their whole extent.

3. The right auricle and ventricle of the heart are still contracting and dilating; the left sinus venosus and auricle move feebly; but the left ventricle is at rest.

4. The right auricle and ventricle are filled with black blood, and the left sinus venosus and left auricle also; but the left ventricle is only about half filled with the same-coloured blood.

5. The trunks and smaller branches of the arteries proceeding from the left ventricle, contain a quantity of this black blood also.

In the early state of medical knowledge, and before anatomy was much cultivated, it was generally supposed, by different writers, that the water produced these effects on animals by rushing into all the cavities of the body, and rupturing some of the organs immediately connected with life (*a*). But since the structure of the body has been more accurately examined, and the use and connexion of the different functions better understood, it appears evident that these changes are to be attributed entirely to the effects

(*a*) Galen. Lib. III. Comment. 40....Ægineta, Lib. III. p. m. 97....Aëtius, Art. Princip. p. 404.....Alexand. Bened. Cap. III.....Codrongus de Submersis, p. m. 322.....Sennertus in Praxi, Lib. II. p. 350.

which the water produces on the lungs; either *directly*, by entering into their cavity; or *indirectly*, by preventing the entrance of the air of the atmosphere.

Several authors have endeavoured to discover by which of these means the water operates; and, after considerable labour and attention, they were greatly divided in their opinions: some contended for the former (*a*), and others for the latter (*b*). The conclusions, however, of both parties were generally drawn either from casual observation, or from insufficient and undigested experiments; in consequence of which neither of the opinions has been established by its partizans; and it still remains a question in physiology, which we shall next attempt to decide by experiment.

SECTION II.

TO DETERMINE WHETHER THE WATER PRODUCES THESE CHANGES DIRECTLY, BY ENTERING INTO THE CAVITY OF THE LUNGS; OR INDIRECTLY, BY EXCLUDING THE ATMOSPHERIC AIR.

IF the water enter into the lungs in drowning, we may naturally expect to find it there afterwards, when the body is opened: but if the body be opened after the obstruction

(*a*) *Platner*, Cent. Quæst. Paradox. p. 35.....*Louis*, Mémoires sur les Noyés, &c. &c.....*De Haen*, Ratio Medendi continuata, tom. 1, &c.

(*b*) *Littreus* and *Senac*.....Histoire de l'Académie Royale des Sciences, anno 1719.....*Haller*, Prælect. Boerhaav. tom. 2. p. 219.....*Winslow*, Experimenta Bruherii.....*Kaaw Boerhaave*, Impetum faciens, p. 228, &c. &c.

of respiration from any other cause, there is often a frothy fluid in the cavity of the lungs, resembling a mixture of air and water, which might mislead the inquirer.

In consequence of this, it has been suggested to add some colouring substance to the water employed in experiments of this kind, that, if it should enter into the lungs in the process of drowning, the peculiarity of colour might serve to distinguish it from this frothy fluid: but if the coloured fluid should enter into the lungs in a small quantity only, the colouring matter may be so much diluted by the pulmonary mucus, as to lose in a great measure its distinguishing character. On this account, it will be better to employ the deepest colours in these experiments, such as very black ink, strong solutions of blue vitriol, &c.

Experiment I.

I filled my glass bell with ink, and immersed a dog in it. As soon as his struggles ceased, he was taken out, and examined. A small quantity of frothy fluid was found in the lungs, and it was coloured with ink.

The same experiment was made on three other dogs; and the fluid found in the lungs of each of them was coloured with ink. It was also repeated on three cats, and with the same general result.

From these experiments it appears, that some of the ink passed into the lungs of these animals; and we may infer from it, that a quantity of water commonly passes into the lungs of animals that are drowned: but it may be

still suspected, that this water enters into the lungs by its own gravity, after the struggles of the animals have ceased; and therefore that the presence of water in the lungs cannot have occasioned the symptoms which preceded.

This question may be decided by putting the bodies of animals into a state similar to that which arises from submersion; and after the struggles have ceased, by plunging them into a coloured fluid: for if the fluid enters into the lungs in this state, it may be inferred, that the ink also entered into them by its own gravity, after the efforts to inspire ceased.

Experiment II.

I passed a ligature round the trachea of a dog, and strangled him. As soon as he ceased to move, I plunged the body into a quantity of ink, and confined it there for several minutes. On examining the lungs afterwards, I found no ink in them.

This experiment was repeated on two other dogs, and with the same result.

Here we find, that the ink does not enter into the lungs by its own gravity, after the efforts to inspire cease. It must therefore have entered into the lungs of the animals of the first experiment during the efforts to inspire.

But if the ink entered into the lungs of these animals during the efforts to inspire, was it not sufficient to produce the changes that took place in the body in consequence of submersion?

To determine this question, we must ascertain the quantity of water that passes into the lungs during submersion, and the changes that are produced on the body by the introduction of an equal quantity of a similar fluid.

If the ink which passes into the lungs would remain there in a separate state, we might ascertain the quantity that passed into them in these experiments, by pressing it out again at the trachea: but this ink is so intimately combined with the mucus of the lungs by the agitation of the thorax, that we cannot collect it in a separate state. If, however, we employ such fluids in these experiments as have no disposition to unite with the pulmonary mucus, they will remain in the lungs in a separate state, notwithstanding the agitation of the thorax, and may be in a great measure collected again.

Experiment III.

I filled a small glass bell with quicksilver, and immersed a cat in it, in the same manner as in the other experiments. When the body was taken out, I found half an ounce of quicksilver (*a*) in the cavity of the lungs, and an ounce of a frothy fluid (*b*) tinged with a red colour.

Three more cats were immersed in quicksilver, in the same manner; and when they were removed, we obtained from the lungs of

	Quicksilver.	*Frothy mucus.*
The first,	three drachms,	six drachms:
The second,	five drachms,	an ounce:
The third,	——————,	an ounce.

(*a*) In weight. (*b*) By measure.

Four rabbits were drowned in quicksilver, in the same manner; and when they were removed, we obtained from the lungs of

	Quicksilver.	*Frothy mucus.*
The first,..........	two drachms,..........	six drachms:
The second,.....	one drachm,...........	half an ounce:

And from the two last we got no quicksilver.

Hence it is evident, that the whole quantity of the fluid found in the lungs of drowned animals is inconsiderable, and that it is generally composed partly of the natural mucus of the lungs, and partly of the fluid which passes into them during the efforts to inspire.

Since, then, the quantity of the fluid which enters into the lungs is so very inconsiderable, are we to suppose that it alone produces the changes which take place in consequence of submersion?

If the quicksilver alone produced these changes in the animals of the last experiment, the same changes will be produced by introducing an equal quantity of any fluid into the lungs of a living animal, when it is not immersed in water.

It was shown, in the last experiments, that the greatest quantity of quicksilver which passed into the lungs of the cats, was five drachms. Let us suppose a quantity equal to the whole of the mucus and quicksilver obtained from the lungs, passed into them from without: if it occasioned the death of the animal, we shall produce the same effect by introducing an equal quantity of water into the lungs of a similar animal, whilst the respiration is not otherwise interrupted.

Experiment IV.

I confined a cat in an erect posture, and made a small opening in the trachea by cutting out one of the cartilaginous rings. Through this opening I introduced two ounces of water into the lungs. The animal had immediately a difficulty of breathing, and a feeble pulse: but these symptoms were soon abated; and it lived several hours afterwards without much apparent inconvenience. At length I strangled it, and found two ounces and a half of water in the lungs.

I introduced two ounces of water into the lungs of two other cats, in the same manner. The difficulty of breathing, and alteration of pulse, were somewhat greater than in the first experiment; but in a few hours these complaints were abated: the animals were then strangled, and four ounces of fluid were found in the lungs.

Here then, although a quantity of water be introduced into the lungs, greater than the whole quantity found in them in these experiments, yet it does not produce in the body those changes that take place in drowning.

It follows, therefore, that the water which enters into the lungs of animals in drowning, is not the cause of the changes that take place in the body.

From these experiments we draw the following conclusions:

1. A small quantity of water commonly passes into the lungs in drowning.

2. The water enters into the lungs during the efforts to inspire, and, mixing with the pulmonary mucus, occasions the frothy appearance mentioned by authors.

3. The whole of this fluid in the lungs is not sufficient to produce the changes that take place in drowning.

And hence it follows, *That the water produces all the changes that take place in drowning*, indirectly, *by excluding the atmospheric air from the lungs.*

To trace these changes to the exclusion of air, we must inquire into the particular effects of the air on the lungs in respiration, and the connexion of these effects with the different functions of the body: and when these are ascertained, it will be easy to determine what changes must take place in consequence of the privation of air.

The first and most obvious effect produced in the lungs by respiration, is an alteration in the volume of air they contain; and this must occasion a proportional change in the degree of their dilatation, and consequently in the form of the vessels distributed upon them.

We proceed to investigate these changes, and to trace their connexion with the other functions of the body.

SECTION III.

TO DETERMINE THE MECHANICAL EFFECTS OF THE AIR ON THE LUNGS IN RESPIRATION.

To obtain the object proposed in this section, we must first ascertain the different quantities of air in expiration and in inspiration, and the proportional dilatation of the lungs in each of these states; and then endeavour to determine the effects of these different degrees of dilatation on the pulmonary vessels, and on the current of the blood circulating through them.

Several writers have attempted to measure the quantity of air taken in by a single inspiration; and some of them have delivered estimates of the proportional dilatation of the lungs, both in inspiration and in expiration (*a*). These estimates were in a great measure adopted by a celebrated physiologist; and he deduced many consequences from them, to explain a variety of diseases immediately connected with these mechanical changes (*b*): but the means employed to form these estimates appear to have been insufficient; and the consequences deduced from them are contradicted by several common appearances of the body: we shall therefore attempt to measure these quantities again, and to form our estimate from the result of the experiments:

(*a*) *Borelli* de Motu Animal. Lib. II.....*Jurin*, Dissert. IV. Lib. IV..... *Hales*, Veget. Statics, Vol. II.....*Sauvage* de Respiratione difficili.....*Bernouilli*, Dissert. de Respiratione.

(*b*) *Haller*, Element. Physiolog.

And, first, to measure the quantity of air taken into the lungs after a complete expiration.

As every animal makes a full expiration immediately before death, the lungs in a dead body are in a state of complete expiration: if, therefore, we can measure the quantity of air in the lungs of dead bodies, we shall determine the medium quantity in a state of complete expiration. Now, it is generally known, that the lungs, in a sound state, are always contiguous to the containing parts of the thorax; and that all the containing parts of the thorax, except the diaphragm, are fixed after death: if, then, we fix the diaphragm of a dead body, and make an opening through the external parts into the cavity of the thorax, the atmospheric air will pass in by its own gravity, and, pressing upon the external surface of the lungs, will force them to collapse, and expel the air they contain; and that part of the cavity of the thorax which they before occupied, will be the measure of the volume of air expelled from them. If we now fill up this part of the cavity with water, it will give the quantity of air which was in the lungs after expiration.

Experiment I.

I procured a dead body of ordinary stature, and applied a close compress upon the superior part of the abdomen, to fix the diaphragm in its situation: then I made a small opening into the cavity of the thorax on each side, and upon the most elevated part. The lungs immediately collapsed; and consequently the air which they contained was expelled. Water was next introduced at these openings, till the cavity was filled; and the contents of the water were 272 cubic inches.

The lungs of this body contained therefore 272 cubic inches of air in a state of complete expiration.

The same experiment was repeated on two other bodies, nearly in the same circumstances; and the lungs of the first, in a state of complete expiration, contained 250 cubic inches of air, and the lungs of the second 262 cubic inches.

The subjects of these experiments died from hanging; and I was not aware that there could be any objection to the experiment on that account: but it afterwards occurred to me, that their lungs were not in a state of expiration; because persons under the influence of fear often make a deep inspiration, before the cord is passed round the trachea; and the pressure of the cord becomes immediately so tight, that they are not able to expel the air again.

On this account I repeated the experiment on several adult subjects, who died a natural death. In some of them the lungs adhered to the sides of the thorax, and did not collapse completely, when the openings were made; but in four of them the lungs appeared to collapse properly; and we had the following results:

The lungs of the	first contained	120 cubic inches,
....	the second	102
....	the third	90
....	the fourth	125

These experiments are sufficient to show, that the lungs contain a considerable quantity of air, even after complete expiration: but this quantity must vary in different subjects in proportion to the capacity of the thorax; it is therefore extremely difficult to establish a medium. However, to

pursue this inquiry, we shall for the present adopt the medium quantity of these latter experiments, and say that the lungs of the human subject contain 109 cubic inches of air after complete expiration.

We next proceed to measure the quantity of air taken into the lungs at an ordinary inspiration.

This may be done by breathing from a vessel which has only two tubular openings, one to be applied to the mouth, whilst the other is immersed in water. If we inspire from the vessel, under these circumstances, a quantity of water will enter it, equal in volume to the quantity of air inspired from it.

On this principle we contrived the machine A B C D E. The vessel D contains several hundred cubic inches of air. This vessel (which for the sake of distinction I call pneumatic) suspended in the balance A B, and brought to an equilibrium with the scale C; and the tube *a b c* being immersed in water in the vessel G; if a person inspires from the tube E, a volume of water will enter the pneumatic vessel through the tube *a b c*, equal to the volume of air inspired from it by the tube E. The scale C, will determine the weight of the water in the pneumatic vessel; and this, by calculation, will give the number of cubic inches of water which entered the machine at a single inspiration, and the number of cubic inches of water will give the number of cubic inches of air.

J. Poupard sculp.t

Experiment II.

An adult person of ordinary stature inspired twice from the machine; imitating, as nearly as he could, an ordinary inspiration.

In the first he took in 3 cubic inches of air;

In the second .... $2\frac{1}{2}$ cubic inches.

Another person, of nearly the same stature, inspired twice:

In the first he took in $3\frac{1}{5}$ cubic inches of air;

In the second .... $2\frac{3}{4}$ cubic inches.

In these experiments there is a difference in the quantity of air taken into the lungs at a single inspiration, which we suspected might arise from the attention of the mind in this new situation of breathing. To avoid this source of error, as much as possible, we determined that the same person should inspire regularly from the pneumatic vessel for a minute or two, and expire alternately into the atmosphere, numerating all the inspirations; and that we should then measure the water which passed into the vessel during that time, and calculate the average quantity for each inspiration.

Experiment III.

The two persons employed in the last experiment inspired from the pneumatic vessel thirty times successively, in the manner just described; and the average quantity of air taken in at a single inspiration,

By the first person, was.......$2\frac{3}{4}$ cubic inches;

By the second,......................$3\frac{1}{6}$.

It appears, then, that the quantity of air taken into the lungs at a single inspiration is very inconsiderable, when compared with what they contain after a complete expiration. This extraordinary difference was remarked with surprise by several judicious friends; and they expressed their fears that I might have been deceived. In consequence of this, we repeated the second experiment again, with still more attention, and on a greater variety of subjects; and in all of them the average quantity of each inspiration was nearly the same as in the preceding: but in the third experiment an anxiety was felt in the breast, before the limited number of inspirations was finished; and when the mouth was withdrawn from the tube, we observed that it was necessary to make a deep inspiration. These two circumstances seemed to show, that the quantity of air taken into the lungs from the machine had not been sufficient for the purposes of respiration; and this deficiency in quantity must be attributed to some particular circumstances of the experiment.

In all these experiments we attended to the *effort* made by the organs of respiration when the ordinary quantity of air is taken in from the atmosphere; and we considered the force of this *effort* as the measure of an ordinary inspiration (*a*), and endeavoured to copy it exactly when we inspired from the machine: but we find that this is not sufficient; because the water must rise into the pneumatic vessel contrary to its own gravity; and in order to overcome this additional resistance, it will be necessary that

(*a*) Every one knows, that there is a considerable difference in the quantity of air taken into the lungs at an ordinary, and at a full inspiration: if a person expires carefully, and then makes a full inspiration, he will frequently take into his lungs upwards of 200 cubic inches of air at a single effort.

the effort to inspire from the machine be greater than the ordinary *effort* in the atmosphere. And this has been the source of the mistake.

Since, then, the effort in the asmosphere is not the measure of a natural inspiration from the machine, we must have recourse to the sensation in the lungs. If we inspire from the pneumatic vessel several times successively, as in the last experiment, and take in as much air at each time as to leave no uneasiness in the lungs during the inspirations, nor any want of a greater quantity when the mouth is removed, we may conclude that the lungs have received as much air at each inspiration as is required for the purposes of respiration.

Experiment IV.

Three persons of ordinary stature inspired from the pneumatic vessel thirty times successively, and took in as much air at each time as the sensations in the breast seemed to require. The average quantity of air taken into the lungs at a single inspiration

By the first, was 12 cubic inches;
By the second,.....14 cubic inches;
By the third,........11.

Hence it appears, that the quantity of air required for a single inspiration is greater than could be expected from the preceding experiments: but this also varies a little in different persons; and it is equally difficult to establish a medium for inspiration, as for expiration. We shall however take the medium quantity here at 12 cubic inches.

But the air which passes from the pneumatic vessel into the lungs, goes from a cool to a warmer temperature; it must therefore undergo a degree of expansion as soon as it gets into the lungs, and consequently occupy a greater space than before. This degree of expansion may be measured, by inclosing a given quantity of atmospheric air in a glass receiver, disposed in such a manner as to indicate the alteration of temperature, and the proportional expansion, at the same time.

Experiment V.

I provided a cylindrical glass receiver, with a thermometer suspended in the middle; I measured the quantity of water the receiver contained, and divided the whole capacity into several hundred parts, which were distinguished by corresponding marks on the outside: then I inverted it in water, and introduced into it an hundred parts of atmospheric air at 69° of Fahrenheit, and applied heat to it gradually by means of warm water, until the thermometer in the inside was raised to 98°: the volume of the whole was then increased $\frac{1}{6}$th. In several successive repetitions the proportional expansion was nearly the same.

If, then, we allow 12 cubic inches for a single inspiration, they will be increased to 14 cubic inches when they get into the lungs; therefore the volume of air before in the lungs receives an addition of 14 cubic inches by an ordinary inspiration. But the volume of air in the lungs, before an inspiration, was 109 cubic inches; hence it will be increased to 123 cubic inches; and the distention of

the lungs must be changed in the same proportion: therefore the dilatation of the lungs after expiration is to their dilatation after inspiration as 109 to 123 (*a*).

We next inquire into the effects of these different degrees of dilatation on the pulmonary vessels, and on the current of the blood circulating through them.

Haller asserts, that the pulmonary vessels are very much changed in the different states of respiration; that they are considerably lengthened by inspiration, and their angles and diameters so disposed, as best to favour the circulation of the blood; but that they are greatly shortened by expiration, and their angles and diameters so much altered, as to produce a complete obstruction to the passage of the blood through them.

" In inspiratione," says he, " pulmo qui nunquam pleu-
" ram deserit, per eosdem passus quibus pèctus dilatatur,
" et ipse in utroque diametro latior nunc fit, et in spatium
" majus, sed sui simile augetur. Id augmentum variè
" estimatum est; quintuplo per inspirationem, pulmonem
" ampliorem fieri clariss. Sauvages conjecit, et inde de-
" cuplo.

" Vasa ergo sanguinea omnis generis cum adtensis
" bronchiis necessariò extenduntur, et flexiones alternæ in
" quas ea vasa in seipsa retracta, in statu pulmonis minimo
" se receperant, eæ nunc in rectitudinem exporriguntur:
" ....Porro quæ sibi incumbebant proxima, ea a mutuo
" contactu discedunt, et anguli inter divisiones vasorum

(*a*) That is, as 4,7769 is to 4,9732; the difference being only, 1963; which is not even two tenths of an inch.

"majores fiunt, spatiaque adeo vicinis vasis interponuntur. "..........Hinc in inspiratione summa facilitas nascitur san-"guini de corde dextro exeunti...........In expiratione vero "pulmo undique urgetur, et in multo minorem molem "comprimitur: vasa ergo sanguinea breviora quidem fiunt "cum retractis bronchiis, eademque angustiora nunc sunt, "siquidem pectus secundum tres suas dimensiones arcta-"tur...........Sanguis ergo quidem in pulmones undique "comprimitur; et venosus æquâ vi pressus, partim versus "arteriosum quidem reprimitur, eumque moratur aliquan-"tum, partim versus cor sinistrum promovetur..........Quare "in expiratione quam ponimus stabilem superesse, pulmo-"nis pro sanguine immeabilitas oritur, quam neque absque "palpitatione, et vitioso conatu, demum omnino ullis suis "viribus cor vincere queat.".................Lib. viii. sect. 4.

But it appears, from these experiments, that the difference in the two states of respiration is much less than Haller has represented; the changes produced on the pulmonary vessels must consequently be so likewise: the inferences he has drawn respecting the circulation of the pulmonary blood are therefore erroneous.

If we suppose the lungs dilated with the ordinary quantity of air in expiration (109 cubic inches), and then suppose 14 cubic inches more to be added, the dilatation of the lungs will be increased; but the increase will be uniform, and only in the proportion of 123 to 109; the pulmonary vessels will be also extended uniformly in all directions (*a*), and their extension will be in the same ratio;

(*a*) To suppose, with Haller, that the angles of these vessels are changed, although the form of the lungs continues the same, is contrary to one of the fundamental principles of geometry.

since then there is no other change produced on the pulmonary vessels by respiration, but a difference in the degree of extension, and since this is so small, the alteration in the sum of their diameters must be equally so; and if the blood circulates through them in one state, it must also in the other: and hence it follows, that the blood circulates through the pulmonary vessels in all the degrees of natural respiration.

But, notwithstanding this conclusion, it may be still suspected that the pulmonary blood does not circulate with proper freedom in all the different states of respiration; that, in the state of expiration, the current may be so much retarded, as to occasion an accumulation in the vessels of the right side of the heart, sufficient to bring on an interruption or suspension of some of the other functions. If it be so, the same effects will be produced by introducing a quantity of any fluid into the cavity of the thorax, to force the lungs to collapse, and expel so much air as will reduce their volume much below the degree of ordinary expiration. This experiment is frequently made in the human body by disease: a quantity of watery fluid gets into the cavity of the thorax between the containing parts and the lungs, occupying a very considerable space, and reducing their volume much below the dilatation of ordinary expiration; and still there is no suspension of any of the other functions. Many examples of this disease have been recorded by different writers, where the fluid was taken away several times from the cavity of the thorax, during the life of the patient: and there is one in the Memoirs of the Academy of Surgery, where the Author expresses much surprise that the blood could circulate through the lungs, while such a quantity of fluid was confined in the thorax.

" Après les symptômes ordinaires," says the Author, " les chirurgiens prononçoient, que c'étoit un hydrothorax, " & décident sur l'opération.

" Le malade étoit donc assis dans son lit, le corps " penché en avant, et soutenu par plusieurs assistans. Je " lui fis la ponction avec un trois-quart ordinaire. Le poin- " çon étant tiré, l'eau sortit par la canule à plein jet, et " par des secousses qui répondoient aux mouvemens de la " respiration. Il en sortit près de six pintes (*a*) d'un fluide. " Bientôt après, son pouls se ranimoit.....Sept jours après, " l'accumulation se faisoit encore; & je tirai par la même " opération encore cinque pintes."

" Dans ces cas, le poumon, fort écarté des parois de " la poitrine, doit être pelotonné vers le centre, & réduit " à un fort petit volume, & ses vésicules très-rétrécies. " ....C'est assez pour expliquer la difficulté de la re- " spiration.

" J'ai observé que toutes les fois qu'on insinuoit la " sonde de poitrine dans la capacité, on l'introduisoit à la " longueur de quatre ou cinq pouces, sans toucher ni ren- " contrer aucune partie intérieure; & c'est une chose qui " m'étonnoit toujours!".............................Tom. II. p. 546.

I have frequently made a similar experiment on several dogs, producing an artificial hydrothorax by introducing as much water through an oblique opening of the intercostal muscles into the thorax, as nearly filled one third of the whole cavity, closing the orifice afterwards. In all of

(*a*) De France, *i. e.* 288 cubic inches.

them it brought on a considerable difficulty of breathing, but no other apparent inconvenience.

In these examples the volume of the lungs must have been considerably reduced, and the greatest quantity of air they were capable of containing much less than the ordinary quantity in an healthy expiration; and still the blood continued to circulate through the pulmonary vessels in sufficient quantity to keep up the action of the left ventricle of the heart, and the other functions of the body.

If, therefore, the blood circulates through the pulmonary vessels with this freedom when the volume of the lungs is so much diminished, it must certainly circulate through them with equal ease in expiration when the volume of the lungs is much greater: hence it follows, that the blood circulates through the lungs in expiration with sufficient freedom to keep up the health of the system.

From all these experiments we draw the following conclusions.

1. The lungs contain 109 cubic inches of air after a complete expiration; and this quantity receives an addition of 14 cubic inches by inspiration.

2. The dilatation of the lungs after expiration is to their dilatation after inspiration as 109 to 123.

3. The blood circulates through the pulmonary vessels in all the degrees of natural respiration.

4. The circulation through them, after expiration, is sufficiently free to keep up the health of the system.

And hence it follows, *that the dilatation of the lungs is not the final cause of respiration.*

Another effect produced in the lungs by respiration, is a change in the chymical qualities of the air they contain. We proceed to inquire into the particular nature of this change, and its connexion with the other functions of the body.

SECTION IV.

TO DETERMINE THE CHYMICAL EFFECTS OF THE AIR ON THE LUNGS IN RESPIRATION.

IT has been long suspected by philosophers of the first character (*a*), that the air which we breathe undergoes some chymical changes in the lungs; and various conjectures have been proposed concerning them, by different writers, at different times; but nothing more could be expected from the most penetrating mind, until chymistry put on the form of a science: from that period the clouds of hypothesis have been dispelled, and the dawn of truth begins to appear. We shall not enter into a detail of these different conjectures, nor of the progressive discovery of the facts connected with the subject; but rather endeavour to describe the constituent parts of asmospheric air, as far as they are now known, and then investigate the particular changes they undergo by respiration.

(*a*) Aristotle, Sir Isaac Newton, &c.

When atmospheric air is analysed with the ordinary chymical tests, it is found to be composed of phlogisticated air, dephlogisticated air, and fixed air. If any given quantity of atmospheric air (suppose 100 parts) be accurately analysed in this manner, it is generally found to contain nearly two thirds of phlogisticated air, one third of dephlogisticated air, and a very small quantity of fixed air: but the proportions of these airs are generally different in different parts of the atmosphere, and sometimes no fixed air can be found.

If 100 parts of analysed atmospheric air be inspired, and expired again into a receiver, it is found to have undergone a change in the proportion of its constituent parts: the quantity of dephlogisticated air is diminished, the quantity of fixed air increased, and the phlogisticated air remains the same.

A celebrated chymist has proposed to ascertain the changes (*a*) which these airs undergo by a single respiration (*b*): but the results of such experiments are subject to some variations from the state of the body, and from the duration of the respiration. Notwithstanding these difficulties, I made several experiments on myself, to ascertain the particular degree of these changes; and although there was always a little difference in the results, yet by frequent repetition they became inconsiderable.

First, I ascertained the proportion of these airs in 12 cubic inches of atmospheric air; then I inspired an equal volume of the same air, expiring it into a glass receiver,

(*a*) Mons. Lavoisier. (*b*) One inspiration and one expiration.

and analysed the whole quantity. This was repeated several times, and the medium quantity was as follows:

The volume of air taken into the lungs at a single inspiration contained,	Parts.	The volume of air expelled from the lungs by the next succeeding expiration contained (*a*),	Parts.
Phlogisticated air...........	80	Phlogisticated air...........	80
Dephlogisticated air.......	18	Dephlogisticated air.......	5
Fixed air.......................	2	Fixed air.......................	13
	100		98

It appears, then, that the diminution of dephlogisticated air, and the increase of fixed air, in a single respiration, is considerable.

Let us next inquire if this diminution of the one, and increase of the other, be uniform and constant in the same volume of air several times respired: and, first, the diminution of the dephlogisticated air.

This may be done by breathing a quantity of air several times from a glass receiver inverted in water, and by mixing a small quantity of this air, after every expiration, with an equal quantity of nitrous air in the eudeometer of Fontana: the quantity of dephlogisticated air will then be indicated by the diminution of volume in the eudeometer.

(*a*) In all these attempts I endeavoured to imitate the natural expirations; but the volume of air expelled from the lungs was never equal to the volume of air taken in; sometimes there was a diminution of $\frac{1}{50}$, and sometimes $\frac{1}{60}$. This diminution was observed long ago by Boyle and Musshenbroek.

I passed 12 cubic inches of atmospheric air into a glass receiver inverted in water, and put a measure of it into the eudeometer, and it occupied 100 parts; then I added an equal measure of nitrous air, and the whole volume of 200 parts was diminished to.. 144

Then I inspired the whole of the volume from the receiver, and expired it in the usual time; and on trying an equal portion of it in the eudeometer, the 200 parts were diminished to............... 158

After the second expiration they were diminished to 163

After the third, to.. 167

After the fourth, to.. 170

After the fifth, to... 171

We might also determine whether the addition of fixed air be constant and uniform in several successive respirations, by breathing a given volume of air in the same manner from an inverted receiver, and passing a small quantity of it through lime-water after every expiration: but this would require great labour and minute attention; and since we know the quantity of fixed air added by a single expiration, it is easy to determine whether there be any additions made to it afterward, by breathing the same air several times, and, after the last expiration, examining it with lime-water, and comparing the whole quantity of fixed air with the known quantity of a single respiration.

I inclosed 12 cubic inches of air in an inverted receiver, and breathed it through a glass tube six times in succession: on being examined with lime-water after the last expiration it contained 15 parts of fixed air. The same experiment was repeated, and the quantity of fixed air was 13 parts.

The diminution of dephlogisticated air, and the increase of fixed air, is therefore constant and successive in the same quantity of air frequently respired: but the changes in the successive respirations bear no proportion to the changes in the first. Since, however, these changes are constant and uniform, they must be connected with some corresponding changes in the lungs equally constant and uniform; and there is no substance in the lungs where we can expect to find such corresponding changes, except the blood which circulates through them.

Lower observed long ago in living animals, that the blood from a wound in the trunk of the pulmonary vein was florid; and knowing before that the blood entering into the lungs by the pulmonary artery is black, he concluded that it acquired this florid colour by passing through the lungs; and finding afterwards that, when the animal ceased to breathe, the blood from the wound in the pulmonary vein was black, he attributed the florid colour to the action of the air in respiration (*a*). This opinion has been mentioned frequently since by different authors, and seems now to be generally believed.

To examine the fact with particular attention, I procured several large dogs, removed the sternum, and exposed the trunks of the pulmonary veins and arteries so as to allow me to distinguish accurately the colour of the blood passing through them. Then I inflated the lungs with a pair of bellows (after the description of (*b*) Vesalius), imitating the natural respiration, and kept the animal alive

(*a*) Tractatus de Corde, p. 185.

(*b*) Vesalius de Corporis Humani Fabrica, Lib. VI. Cap. xix. p. 572.

by this process for a considerable length of time. In these experiments it was observed, that, during the inflation, the blood in the trunks of the pulmonary artery was black, but in the trunks of the pulmonary veins it was florid: and when the inflation was intermitted for a minute, the blood in the trunks of the pulmonary vein became gradually black, like that in the arteries.

In some of these animals I divided the trunks of the humeral artery and vein; and during the inflation the blood from the artery was florid; but, on intermitting the inflation, it became gradually black, like the blood from the vein.

I also examined these appearances in the toad and the lizard, whose lungs consist of only a transparent bladder, with blood vessels so thin that the colour of the circulating blood may be easily distinguished through them.

I inflated the lungs of these animals several times, and emptied them again by gentle pressure, imitating by this method the respiration of the more perfect animals; and in all the examples, when the atmospheric air passed into the lungs, the blood in the pulmonary vessels became gradually florid; but when they were emptied, it became gradually black: to this may be added the familiar circumstance in blood-letting, the dark coloured blood becoming florid after exposure to the air.

All these facts seem to confirm the opinion of Lower, that the blood acquires a florid colour in passing through the lungs; and that this colour is produced by the chymical action of the air.

We next inquire by what part of the respired air this change of colour is produced, and what particular chymical process takes place at the time.

Does it arise from the addition of fixed air separated from the blood in passing through the lungs, or from the chymical action of the phlogisticated or the dephlogisticated air?

If the florid colour arises from the separation of fixed air in its entire state from the blood, then florid blood exposed to a volume of fixed air in a close vessel must become black also.

I introduced four ounces of florid blood, fresh drawn, into a glass receiver containing fixed air, and I confined it there a considerable time; but it did not become black, nor undergo any sensible change. I then received some florid blood from the carotid artery of a sheep into a phial filled with fixed air; but still the florid colour was not altered: therefore the change of colour in respiration does not arise from the separation of fixed air in its entire state from the blood.

Nor can it be attributed to the chymical action of the phlogisticated air; for it is generally known, that black blood, fresh drawn, exposed to phlogisticated air in a close vessel, undergoes no change of colour.

But it has been often asserted, that black blood fresh drawn, and exposed to dephlogisticated air, becomes florid.

To ascertain the truth of this assertion, I inclosed a quantity of dephlogisticated air in a glass receiver inverted

in quicksilver, and introduced into it four ounces of blood fresh drawn from the jugular vein of a sheep: the blood became instantly very florid, and the quicksilver seemed to ascend a little in the receiver. To ascertain this latter circumstance, I repeated the experiment three or four times: the change of colour was always very sudden, and after several minutes the quicksilver ascended two or three lines. It is evident, then, that dephlogisticated air changes the colour of black blood, and a small portion of the air disappears in the process: but, as the changes in these experiments are similar to those in respiration, it might be inferred, that dephlogisticated air produces the florid colour in both examples.

To satisfy myself of this, I inflated the lungs of several kittens with dephlogisticated air, after the sternum was removed; and the blood in all the pulmonary veins became immediately very florid.

Hence it is evident, that the florid colour acquired by the blood in respiration is produced by dephlogisticated air. But it may be asked, by what means the air is applied to the blood in respiration? whether by means of the absorbents, or by the force of chymical attraction?

If it were taken in by the absorbents, it must pass directly to the right side of the heart, and change the colour of the blood there; which does not happen.

It has been shown by Dr. Priestley, that atmospheric air will change the colour of blood through the coats of a bladder; but there are no direct proofs that it will produce the same effect through the coats of the vessels in a living animal.

To determine this, I dissected the cellular membrane from the small veins in the neck of several rabbits, and confined the blood in them by ligatures; then I conducted a small stream of dephlogisticated air to the coats of the vessels: in some of them the blood became a little florid; but in others there was no distinct change, although the stream of air was conducted for two minutes.

In these instances, however, where it took place, something must have pervaded the coats of the vessels; and this makes it highly probable, that, when the dephlogisticated air produces the change of colour in the blood, something pervades the coats of the pulmonary vessels by the force of chymical attraction.

But what it is that pervades the vessels, is not yet known: whether some principle separated from the blood be combined with the dephlogisticated air, to form fixed air; or whether the dephlogisticated air be decomposed, and a part of it passes into the blood, while the other part remains behind in the form of fixed air; or, lastly, whether the dephlogisticated air enters into the blood in its entire state, whilst the fixed air is separated from the vessels?

The greater number of facts in chymistry seem to favour the first supposition; but yet they only make it probable; and more than probability cannot be expected, until the nature of these airs be better understood.

But, whatever solution future experiments may furnish, this general fact will remain unaltered; viz. That the change of colour which the blood undergoes in passing through the lungs, is occasioned by the chymical action of

the dephlogisticated air of the atmosphere; and that, in consequence of this, fixed air is received.

To proceed in the inquiry, we endeavour to trace the connexion of this change in the pulmonary blood with the other functions of the body.

It is generally known, that when an animal breathes the same air several times, his pulse becomes slower, till at length it ceases, and all the other functions are suspended; and it is also known, that when respiration is obstructed, symptoms very similar are produced. How are these facts connected together?

It has been shown already, that, when the same air is breathed several times, or when it is retained in the lungs longer than usual, there is a gradual diminution of dephlogisticated air, and an increase of fixed air. These symptoms then must arise from one of these circumstances; either from the noxious quality of the additional fixed air, or from the want of the salutary quality of the absent dephlogisticated air: but the additional fixed air exerts no noxious quality in the lungs; because it may be breathed in a much greater quantity without any inconvenience when mixed with atmospheric air: they must then be attributed to the gradual diminution of the dephlogisticated air.

When the dephlogisticated air is thus diminished, the ordinary change of colour which the blood undergoes in the lungs will be also diminished, till at length it will pass through the pulmonary veins with the same black colour as when it entered into the arteries.

This has been already in a great measure demonstrated in the examples of artificial inflation; and it may be seen still better in the toad and the lizard, where the lungs may be exposed a long time without destroying the life of the animal.

I forced a considerable quantity of air into the lungs of a small dog, whose sternum had been removed, and confined it there by a tight ligature round the trachea: the blood, continuing to circulate through the lungs in this state, began immediately to put on a shade of brown in the trunks of the pulmonary veins; and in less than two minutes it became very black.

With the same view I provided a large toad, and exposed his lungs on both sides; and while they were filled with air, I immersed him in a small glass receiver containing water. When he was first put into the water, the blood circulating through his lungs was florid: when he had continued in the water twenty minutes, the lungs still filled with air, the blood in all the pulmonary vessels became gradually dark-coloured, till at length it was black. This was repeated several times with the same animal, and once or twice also with lizards; and in all the instances where they retained the air in their lungs a long time after immersion, the pulmonary blood became gradually black.

It follows, therefore, that when the dephlogisticated air is gradually diminished, the blood passing through the vessels of the lungs does not undergo the changes of colour which take place in ordinary respiration; and that the symptoms which follow obstructed respiration must be attributed to this particular quality of the blood. But by what means it produces these symptoms, we next inquire.

The first supposition is, that it exerts some noxious quality on the nerves of the lungs, which is communicated by them to the brain.

If it operates in this manner, the same symptoms will not be produced by obstructed respiration, when the nerves distributed on the lungs are so divided that their communication with the brain is intercepted.

To ascertain this, I provided a small dog, and divided the trunks of the par vagum and great intercostal nerves on each side of the neck about an inch below the larynx: the skin was then sewed up, and the animal appeared to suffer no other inconvenience than a laborious respiration. The next day I put him under a glass bell inverted, and filled with atmospheric air. When he had continued there several minutes, the respiration became difficult; and soon after he fell down without any sign of life. I divided the nerves of another dog in the same manner; and the day following I passed a ligature round the trachea, and obstructed the respiration: soon afterward he fell down without any sign of life.

It appears, then, that the black blood produces the same symptoms when the nerves of the lungs have no immediate communication with the brain; and hence, in the cases of obstructed respiration, it can exert no such noxious quality on the brain by the mediation of the nerves.

From the lungs this black blood passes immediately into the sinus venosus and left auricle of the heart. What changes does it produce there?

The only sensible change it can produce in the heart, is in its contractions; and these may be easily observed by inflating the lungs, when the sternum is removed, and the pericardium opened in such a manner that the motions of the auricles and ventricles may be distinctly perceived.

I made these experiments with all the circumstances here mentioned; and in the process of inflation I attended carefully to the changes in the colour of the blood, and in the corresponding contractions in the left auricle and ventricle of the heart; and in all the examples, I observed that when the blood which passed into the left auricle was florid, the auricle and ventricle contracted strongly, and the circulation went on as in health; but when the blood began to put on a shade of brown, the contractions were diminished; and when it was black, the contractions ceased, although the auricle was distended with blood; and as the contractions ceased, the functions of the body were suspended; but as soon as the florid colour began to be restored, the auricle and ventricle resumed their contractions, and they gradually recovered their natural state; and all the other functions returned.

In these examples the contractions of the left auricle and ventricle are immediately affected by the quality of the blood (*a*) passing into them; for when they cease, the

(*a*) This explanation has been objected to by some persons, on very respectable authority. It has been said, that the heart resumes its contractions at the first inflation; and, as the blood in its cavity cannot be supposed to be changed so soon, the renewal of the natural contractions must arise from some other cause. But these two circumstances are only assertions, which have not yet been demonstrated. Besides, some contractile motion may be produced in the heart by the agitation it suffers from the first distention of the lungs, in the same manner as feeble contractions are

auricle is filled with black blood: but notwithstanding these facts, to such as have particular opinions about the manner in which the blood acts upon the heart, it may not appear clear that these alterations in the contractions arose from the quality of the blood alone. This difficulty, however, may be removed by attending to similar experiments in the amphibious animals (*a*), where the heart has only one

produced in the hearts of amphibious animals out of the body by agitation, or gentle compression; but these motions are not so strong as the natural contractions, and they ought to be carefully distinguished from them.

(*a*) Some respectable writers have employed the word *sympathy*, to express or explain the connexion between the heart and lungs. When the lungs are inflated, the heart moves, they say, by sympathy; but, as they have not defined the sense in which the term is here employed, no one can judge of the propriety of their reasoning. If by sympathy they mean here a separate principle or quality that is supposed to lie hid in the body, it is only a particular name for one of the occult qualities of the peripatetic school, which cannot be proved to exist; and, as they convey no information to the mind, they have been long since banished from all chaste philosophy.

If the term be intended to express a mechanical cause, to assert that the motion of the lungs in respiration keeps up the natural contractions of the heart mechanically, the assertion is not supported by facts: for, if it were so, the motion of the lungs would be alone sufficient to keep up the contractions of the heart, and any kind of aërial fluid would be then equally effectual in respiration; and whenever the motion of the lungs ceased for a while, the contractions of the heart would always cease also: but there are several kinds of aërial fluids which are not sufficient for the purposes of respiration; for when animals breathe them separately, the contractions of the heart cease, even while the respiration continues; and when the amphibious animals are immersed in water, the motion of the lungs ceases, yet the natural contractions of the heart continue upwards of an hour afterward; which could not happen, if the motion of the lungs occasioned these contractions mechanically.

But probably the term is used here in its original and more confined signification; *i. e.* to express the co-existence of effects, or the constancy with which one change in the body follows or accompanies another, without any regard to the efficient cause of either, or to the manner or probability of their connexion. In this sense, as it expresses only the fact, no one can object to the use of it: but I cannot, with propriety, employ the word in this inquiry, as my object is to trace the manner of this connexion between the lungs and the heart, and to fill up the chasm by investigating their proper causes.

auricle and ventricle, where the pulmonary artery is a small branch from the aorta, and the pulmonary vein proportionally small empties itself into the sinus venosus and auricle along with the vena cava ascendens, which carries the principal part of the blood. Here the quantity of blood brought by the vena cava would be sufficient to keep up the action of the heart, independent of the pulmonary circulation, if quantity alone were required; and the coats of the sinus venosus and auricle, as well as those of the blood-vessels, are almost transparent: the lungs also contain a quantity of air sufficient to furnish the necessary chymical changes to the pulmonary blood for a considerable time without any communication with the atmosphere; so that the alterations which take place in the colour of the blood, and in the motions of the heart, in consequence of obstructed respiration, are more gradual and distinct than in the animals with double hearts, in which all the blood passes through the lungs.

I confined a large living toad on a plate of metal, with his belly upwards; then I removed a part of the sternum, and his heart and lungs were exposed to view. The lungs were then filled with air; the blood in the pulmonary veins was florid, and the heart contracted 44 times in a minute. In this state he was immersed in a small quantity of transparent water, where the alterations in the colour of the blood, and in the contractions of the heart, could be accurately distinguished. When he had remained in the water 15 minutes, the blood in the lungs began to put on a dark colour, and the contractions of the heart were diminished to 30. In 15 minutes more the dark colour of the blood was increased, and the contractions of the heart were 18. The animal now made several struggles to re-

lieve itself, and threw some air out of its lungs; but the pulmonary blood becoming still more dark-coloured, the contractions of the heart were diminished still further, and in 40 minutes more they ceased; although the sinus venosus and auricle, and the trunk of the vena cava, were filled with black blood. The animal was now removed from the water, without any signs of life; but before the expiration of two minutes he opened his mouth, and took a large quantity of fresh air into his lungs. Soon after, he emptied them almost entirely; and this was repeated several times. During the process, the blood in the pulmonary veins began to be florid, and the heart to renew its contractions; and in 15 minutes from the first inflation the contractions of the heart were 35, all the functions were recovered, and he walked about without any expressions of uneasiness.

This experiment was repeated several times on the same toad; and sometimes, when he did not fill his lungs, after being removed from the water, we injected some air into them with a blow-pipe, and pressed it out afterward, in imitation of the example he had given us; and by these means we renewed the contractions of the heart several times. But at length he was removed from the water, and suffered to remain an hour before fresh air was injected into his lungs. Soon after it was injected, the blood in the pulmonary veins became florid; but the heart did not again renew its contractions.

These circumstances were repeated afterward in the lizard; and in all the examples the contractions of the heart were diminished in frequency, in proportion as the blood from the lungs became dark-coloured; and they were renewed, when it became florid, in the same manner as in the experiment with the toad.

In all these examples the quantity of blood which came to the heart was fully sufficient to keep up its contractions; and still they were gradually diminished in proportion as the pulmonary blood became darker; and when it was black, they ceased entirely. These alterations, therefore, in the heart's motion, must arise from the quality of the blood only.

But it may be questioned, by what means the quality of the blood can diminish the motion of the heart? It must either exert some noxious power on the heart itself, or it must be an insufficient stimulus to excite its contractions.

If it exerted a noxious power upon the heart itself, it must diminish or destroy the faculty of contraction (*a*); and if it diminished or destroyed the faculty of contraction, the heart would not contract again when those means are applied which are necessary to produce the contractions: but in all these experiments, where the heart ceased to contract when the blood which passed into it was black, as soon as some air had been received into the lungs to change a part of this blood to a florid colour, the contractions of the heart were renewed, and soon returned to their natural state.

Therefore this black blood exerts no noxious power upon the heart itself; and we must conclude that, in all these cases of obstructed respiration, the heart ceased to contract, because the blood which passed into it was an

(*a*) By "faculty of contraction in the heart," I mean that quality by which it is disposed to propel the blood through the circulating system.

insufficient stimulus (*a*). And hence it follows, *That the chymical change which the blood undergoes in the lungs by respiration, gives it a stimulating quality, by which it is fitted to excite the left auricle and ventricle to contraction.*

From all these experiments we draw the following conclusions:

1. A quantity of dephlogisticated air is separated from the atmospheric air in the lungs by respiration, and a quantity of fixed air is added to it.

2. The dephlogisticated air exerts a chymical action on the pulmonary blood; in consequence of which it acquires a florid colour.

3. In ordinary respiration this florid colour is seen distinctly as the blood passes into the left auricle; and the heart contracts then with its natural force and frequency.

(*a*) This conclusion may perhaps, at first, appear singular; as the same black blood is a sufficient stimulus to the right auricle and ventricle; because it is expelled from them just before, when it enters into the lungs: and if it be a sufficient stimulus to excite the contractions of the heart on the right side, why not on the left also?

But it must be remembered, that the two sides of the heart do not resemble each other exactly in all their qualities: there is a considerable difference between them, *both in respect to the quantity of muscular fibre, and the facility of being excited to contraction;* and this alone is sufficient to disarm the objection.

But, even if there were no such difference, the instance is not peculiar to the heart; there are several examples in the animal body, where muscles of a similar structure are not excited to contraction by the same or a similar power: some of the muscles are thrown into contraction by the will; some, by particular conceptions of the mind; and some, by chymical stimuli: yet none of these different powers will produce a perfect contraction of those muscles to which they are not adapted by nature. Objections of this kind, therefore, do not apply immediately to our conclusions, but rather to the general law in animal bodies, which must be regarded at present as an *ultimate fact.*

4. When respiration is obstructed, the florid colour is gradually diminished, and the contractions of the left auricle and ventricle soon cease.

5. This cessation of contraction arises from a defect of a stimulating quality in the blood itself.

And hence it follows,

That the chymical quality which the blood acquires in passing through the lungs, is necessary to keep up the action of the heart, and, consequently, the health of the body.

SECTION V.

TO DETERMINE THE NATURE OF THE DISEASE PRODUCED BY SUBMERSION.

IT has been shown, in the first section, that animals confined in water throw out small quantities of air, and then endeavour to take some in from the atmosphere. By these efforts a portion of the surrounding fluid passes into their mouths, and often descends into the lungs; but the quantity is not sufficient to produce the symptoms which afterward appear: (see the experiments in the second section) in the cavity of the lungs it mixes with the air, and increases the dilatation of the air-cells; in consequence of which the lungs are in a middle state of dilatation; that is, between expiration and inspiration: and in this state the blood circulates through the pulmonary vessels with suf-

ficient freedom to keep up the health of the system; (see the conclusions in the third section) therefore the symptoms are not produced by an obstruction of the circulation through the lungs.

From the situation of the animal, the air and water are confined in the lungs; and the dephlogisticated air which it contains is gradually consumed; and consequently the blood passing through the pulmonary vessels receives less and less of its florid colour, and the contractions of the heart become proportionally slower, until they cease entirely (see the experiments and conclusions of the fourth section), although the faculty of contraction still remains.

It appears, then, that the cessation of the heart's motion may be fully accounted for from the action of the water preventing the entrance of the air into the lungs.

Let us next inquire, if the other particular symptoms (see the first section) can be traced to the obstruction of respiration, and to the consequent cessation of the heart, as their proper cause.

In proportion as the colour of the blood passing through the lungs is darker, the contractions of the left auricle and ventricle, and the *corresponding pulsations of the arteries, become weaker*, and the current of the blood slower; and whilst the blood moves slowly in the larger trunks, it begins to stagnate in the smaller branches of the arteries and veins, where the resistance to its passage is greatest. At length, when the pulmonary blood is no longer fitted to excite the sinus venosus and auricle to contraction, *they receive it into their cavity, and remain at rest.* As soon as

they cease to contract, and propel the blood to the head, *all the intellectual operations cease* (*a*), *sensation and voluntary motion are suspended, and the external signs of life disappear;* and the black blood remaining at rest in the arteries, and particularly in the smaller branches of the arteries and veins, occasions the *blue colour upon different parts of the body* (*b*), *but especially about the face and lips*,

(*a*) Whatever may be the efficient cause of the intellectual operations, sensation, &c. the circulation of the blood through the brain is an indispensable condition for them in all the more perfect animals. This is proved by the familiar examples of fainting from sudden or great losses of blood; where, as soon as the motions of the heart can be perceived to cease, consciousness, and all the other operations attributed to the mind, disappear; and when the action of the heart is restored, they all return again.

(*b*) It appears from the dissection (see Sect. V.) that this particular blue colour of the face, &c. arises from the presence of the black blood in the smaller branches of the arteries and veins; and it is still further confirmed by the following remarkable case in the Observationes Anatomicæ of Sandifort, of the authenticity of which I have abundant confirmation from living testimony. The subject is a boy born in 1764.

" In the second year of his age the nails of his fingers were observed to become blue; but this blueness often disappeared, and returned again, for the space of a year, when a general languor came on, and some livid spots were observed on his face, which increased very much by exercise, till soon after the whole face became blue on the smallest exertion of the body, and particularly the lips and tongue. Towards the latter end of the year he perceived an anxiety about his breast, and complained of a general coldness of the body, which was not relieved by the application of external heat. Blood-letting relieved the anxiety in the breast: his blood was very black, and did not separate properly into serum and crassamentum. In the third year he perceived frequent palpitations of the heart, for which the cold bath and exercise were recommended; but they seemed to aggravate his complaints. These symptoms continued stationary until the 10th year, when they were considerably increased, and he had a spitting of blood.

" In the eleventh year he perceived an anxiety in his breast upon the slightest motion of the body, and he frequently fainted: the face also seemed to be a little swelled, and the blue colour was much increased. At length his legs became œdematous, and he died. On opening the thorax, it was found that the aorta took its origin from both the ventricles of the heart; one and a half of the valvulæ semilunares were extended over the right ventricle, and one and a half over the left ventricle; so that half the quantity of blood which passed into the aorta was always black, not having passed through the lungs to undergo the alteration of colour."

where the number of superficial vessels is most considerable. After the left auricle and ventricle have ceased to contract, the right auricle and ventricle are still exposed to the action of those means which naturally excite them to contraction, *and they continue to contract for several minutes*, and propel the black blood into the pulmonary arteries: this occasions an accumulation in the pulmonary vessels, and a livid colour on the lungs: but from the resistance to the entrance of the blood into the pulmonary arteries, and from the want of the synchronic assistance of the left ventricle, the contractions of the right become gradually more feeble, until at length they cease; and the right auricle, being soon after greatly distended with blood, ceases to contract also; yet the faculty of contraction still remains.

Thus we have traced the connexion of the symptoms, and the consequent cessation of the heart, to the obstruction of respiration, as to their *proper cause*. It remains only now to ascertain the name of the disease, and the place it should have in nosology.

As the gradual diminution of the pulse is a constant symptom of this disease, it was first called Syncope (*a*): but, from some peculiarities afterwards observed, physicians thought proper to distinguish it from the milder kinds of Syncope, and called it Asphyxia (*b*); and this name has been generally retained. But another difficulty arose

(*a*) Syncope.....Motus cordis imminutus, vel aliquamdiu quiescens.... Nosol. Cullen. Genus 64.

(*b*) Ασφυξία. Deletis omnibus vitæ indiciis, accedente etiam suffocatione, mortis imaginem ita refert ut merìtò dubitetur, vitamne, an mortem prædicare fas sit.........Inst. Pathol. H. D. Gaubii.

about the place it should have in nosology; and, on account of the obscurity of its nature, it was for some time excluded from every system. At length it appeared probable, from the general mass of observations and experiments, that the diminished action of the heart and arteries arose from a morbid state of the brain, occasioned by the pressure of some blood. In consequence of this, the primary affection was called Apoplexy, and the diminished action of the heart only a symptom; and the disease was placed under the genus Apoplexia, where it still remains (*a*). If, however, we are to consider the primary morbid affection of the body as the disease itself (*b*), and all the consequent appearances only as symptoms; this disease is in the blood, and consists in the presence *of this black blood in the left side of the heart and arterial system;* and the diminution of the action of the heart and arteries, and the blue colour of the face, &c. &c. are only symptoms: this disease, therefore, might with more propriety be named Melanæma (*c*), and classed with others which have some resemblance to it.

As there is no *Pyrexia* here, nor any *primary Neurosis*, and as the alteration of colour in the skin is a constant symptom, would it not be more consistent with the principles of nosological arrangement to place it in the class Cachexia, and order, Impetigo? And since there is no genus for these diseases, may we not propose one with this name and definition?

(*a*) Synopsis Nosolog. Cullen. pag. 190.

(*b*) Status ille corporis humani viventis, quò fit, ut actiones homini propriæ non possint apposìtè ad leges sanitatis exerceri, Morbus dicitur.

Inst. Pathol. H. D. Gaubii.

(c) Ex *μέλαν ᾿αιμα*, sanguis niger.

MELANÆMA,
impedita sanguinis venosi in arteriosum conversio, cujus signa, Syncope, et livor cutis.

And this genus would afford an asylum to several nosological wanderers that have not yet found a permanent resting-place: for it is a natural consequence of these experiments, that the diseases which are brought on by hanging (*a*), by breathing fixed or phlogisticated air, are all produced by this black blood going into the heart unchanged by respiration; and they should be therefore placed in the same family (*b*): but, as name and arrangement are only of secondary importance, when the nature of a disease is ascertained, it may perhaps be more agreeable to general prejudice to retain the accustomed title Asphyxia, and to arrange it as a symptomatic Syncope, with something added to express the primary affection: as

Asphyxia a sanguine venoso in auriculam et ventriculum sinistros transeunte.

These, however, are only proposals for the consideration of nosologists, which we willingly leave to their future determination.

(*a*) It is somewhat extraordinary that the greater part of practitioners still suppose the death, which follows hanging, to be produced by compression on the brain; notwithstanding the number of facts in surgery which show, that a pressure on the brain, much greater than could be supposed to arise from any quantity of blood accumulated there by hanging, will not diminish the contractions of the heart immediately, nor often for several hours, and even days, afterward.

(*b*) Perhaps in this manner:
Melanæma a Submersione,
..................a Suspensione,
..................ab Inspiratione Aëris fixi,
..................ab Inspiratione Aëris phlogisticati, &c.

SECTION VI.

TO DETERMINE THE CONDITION OF THE BODY IN THIS DISEASE, AND THE MEANS OF DISTINGUISHING IT FROM DEATH ITSELF.

OF animal bodies there are only two general conditions, life, and death; and since by *death* we understand the privation of life, there can be no intermediate state between them. Of the body in this disease, we can say with propriety only that it is alive, or that it is dead. If it were really dead, it would necessarily follow, that the means, which are employed to recover it, in the different experiments of the fourth section, must be supposed to communicate life to dead matter, which is impossible: the body is therefore alive, but with a degree of life less perfect than in the ordinary state of health; and since a difference in degree does not occasion an alteration in kind (*a*), the body must still retain that principle which is the immediate cause of all the functions, that are performed in health (*b*); only it is not now excited to action; because the external concomitant circumstances, which operated upon it in health, are removed. These external circumstances are heat and respiration.

(*a*) Majus aut minus non variat speciem.

(*b*) Hence it appears, that there is a striking impropriety in the terms, which are commonly employed to express the state of the body in this disease, viz. "*suspended life,*" "*suspended animation,*" &c. and these terms ought to be laid aside; because they lead mankind to believe themselves capable of *reanimating* or *resuscitating* a lifeless mass, when they only cure a disease.

To ascertain the seat of this principle, we enquire into the effects of the privation of heat and respiration on the living body.

It is generally known, that a small diminution of the ordinary temperature of the body will not occasion a suspension of any of its functions: but it is also known, that a considerable diminution of the ordinary temperature of the body will occasion a suspension of the greater part of its functions (*a*). A certain degree of heat, therefore, in living bodies is indispensably necessary to keep up the functions of health. But although the heat be indispensably necessary, it is not in itself sufficient to keep up these functions without respiration: for if heat be applied to the living body, when the different functions are suspended, none of them will be recovered, until the respiration be established; and the respiration is not always established in this state, in consequence of the application of heat, but often requires artificial assistance (*b*). (see the experiments with the amphibious animals in the fourth section.)

Since, then, the presence of heat, in the living body is not in itself sufficient to keep up the different functions without respiration, the heat must not be regarded as the whole cause of the continuance of these functions, but rather as putting the body in a condition to produce them, when the effects of respiration are exerted upon it. And

(*a*) Flora Siberica, præfat. p. 72.

(*b*) No one can deny that the respiration is sometimes established in this state, in consequence of the application of heat. Reaumur mentions a case of this disease, where a person was recovered, only by being exposed to the rays of the sun: and nature seems to employ the same means to recover the hybernating animals from their torpor, a state very similar to this disease.

hence, when the natural temperature is considerably diminished in this disease, the body is deprived of a condition indispensably necessary to favour the operation of respiration in restoring the functions of health.

From the want of respiration the heart ceases to contract; because the blood, which passes through the lungs, is an insufficient stimulus to it; and the heart ceasing to contract, all the other functions are suspended. But if the temperature and respiration be restored to the body soon after the cessation of the heart, the natural contractions will be renewed, and all the other functions recovered. And since the contractions are renewed by the application of the ordinary stimulus, the heart retains that principle (*a*), which is the immediate cause of its contractions, even after all the other functions are suspended.

But if we attempt to restore the temperature and respiration to the body a considerable time after the cessation of the heart, its contractions will not be renewed, nor will any of the functions be recovered: and when the contractions are not renewed by the application of the ordinary stimulus, the heart has lost that principle, which is the immediate cause of its contractions, and we have no means of restoring it.

From these facts it is evident, 1st, That the heart is the great seat of the principle of life in all the more perfect animals; 2d, That the contraction (*b*) of the heart with the ordinary stimulus, is the only mark of the presence of this principle; and when the heart contracts under these circum-

(*a*) The principle of life.

(*b*) That action of the heart, by which it exerts the ordinary force to propel the blood along the circulating system.

stances, the body is alive; but when it does not contract under these circumstances, the body is dead: life in the more perfect animals may therefore be defined, *The faculty of propelling the fluids through the circulating system.*

And whenever the functions of an animal are suddenly suspended, and the body puts on the appearance of death, it is always in our power to determine whether it be really dead, by restoring the temperature, and by inflating the lungs with proper air. But, to render the decision complete, it will be necessary to regulate their application by a particular attention to the state of the lungs, and to the immediate object of respiration. These shall be considered in the next section.

SECTION VII.

TO DETERMINE THE BEST MEANS OF CURING THE DISEASE.

THE means of curing this disease are so much anticipated in different parts of this essay, that nothing remains to be added here, but a few observations on the manner of conducting the application of them.

To recover the suspended functions, we must renew the contractions of the heart; (see section the fourth;) and this can be done by restoring to the body its heat and respiration: (see section the sixth.)

In every case therefore of this disease, the only object in the cure is to excite the contractions of the heart; and the only means necessary to be employed are the application of heat to the body, and air to the lungs: but as the operation of these powers will be more or less effectual, in proportion as they are regulated by a regard to the circumstances of the body, it will be necessary to deliver some particular directions for the application of them.

When we attend a person in this disease, the first business is to examine the temperature of the body, and, if it be considerably below 98°, to order the application of heat: but, since the scale of heat is so very extensive, it is necessary to determine the degrees of it best calculated to promote the recovery.

It is generally known from several familiar facts, that whilst the circulation of the blood continues, the temperature of the body may be raised many degrees above the natural standard without destroying the principle of life: but it appears on the other hand, from the result of many attempts to recover the hybernating animals from their torpor, that when the circulation of the blood has ceased, and the temperature of the body is reduced near the freezing point, if heat he applied either very suddenly, or in a very high degree, the principle of life is soon destroyed; whereas, if it be applied gradually, and in a very low degree, to the same animals in the same circumstances, the principle of life is often excited to action, and the functions are soon restored.

The effects of heat therefore are extremely different in different states of health and disease; and the greatest

caution is required in deducing any inference from analogy to regulate the application of it as a remedy.

But since the condition of the body in this disease is nearly the same as in the state of torpor, and since the progression of the recovery is also the same ; it can scarcely be unsafe to conclude, that heat will produce the same effects on the body in this disease, as in the torpid animals.

To favour the recovery then most effectually, the application of heat should be conducted on the same plan, which nature has pointed ont for the torpid animals. It should be applied very gradually and uniformly ; and it may be raised to 98°, but not further than 100°.

When the body is warmed uniformly, and the heat of the interior parts about 98°, we direct our attention to the state of the thorax ; and if the patient make no attempt to inspire, we proceed to inflate the lungs with air.

When a person is in health, the object of respiration is to change the quality of the blood passing through the pulmonary vessels, and to fit it to excite the contraction of the left ventricle of the heart: but in this disease the pulmonary veins, the sinus venosus, and auricle, contain a quantity of blood, which has passed through the lungs without undergoing this necessary change: the first object therefore in inflating the lungs is to change the quality of the blood in the trunks of the pulmonary veins, sinus venosus, and auricle, to fit it for exciting their contractions. This must be done by introducing the air so deep into the

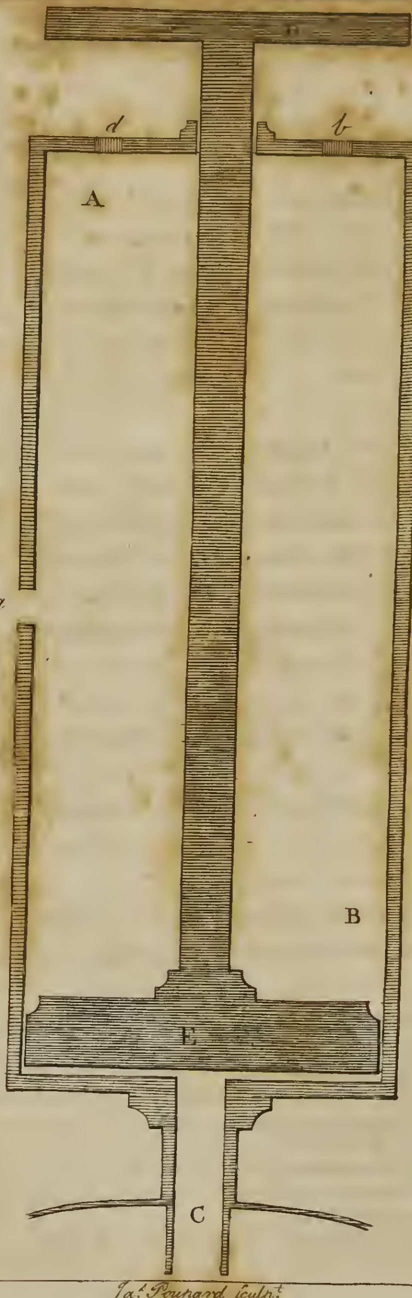
lungs, that it may exert a chymical action on the blood in these cavities.

For this purpose, a great quantity of air must be introduced at each inflation; for if only 12 cubic inches be injected at a time, this small quantity will occupy the larger branches of the trachea, and consequently only a small number of the pulmonary vessels will be exposed to its action: but if a much greater quantity of air be forced in at each time, some of it will pass into all the more remote cells; and when they are thus uniformly distended, the pulmonary veins, the sinus venosus, and left auricle, will be exposed as much as possible to its action, and some of the altered blood may be forced into them from the smaller vessels.

On this account we should introduce upwards of 100 cubic inches of air into the lungs of an adult at each inflation (*a*), and it must be carefully drawn out again before more be introduced.

But one difficulty still remains: there is sometimes a quantity of water insinuated into the small branches of the trachea, and into the air-cells; (see the second section) and if the lungs be inflated in this state, the water will occupy those places where the fresh air is principally required: and although the lungs be inflated with the utmost care and attention, it may be impossible, on this account, to apply the fresh air, sufficiently near the sinus venosus

(*a*) This direction is further confirmed by the circumstances of spontaneous recoveries from an ordinary Syncope: the first inspiration is generally very full.



Ja.s Poupard sculp.t

and left auricle, to change the quality of the blood they contain (*a*): in all cases, therefore, where the quantity of water is considerable, we must remove some of it, before the air is introduced. Sometimes a small portion of it will pass out by its own gravity, when the head is reclined; and still more may perhaps be taken away by an instrument so contrived, on the principles of the pump, as to exhaust the lungs of a part of their contents.

For this purpose, I propose the instrument A B C D E. The brass cylinder A B contains 100 cubic inches of air, and communicates with the atmosphere by the small circular opening *a*. The piston D E is of wood, and lined with a soft substance at the bottom E, so as to be air-tight. The two openings *d*, *b*, are to allow the air to escape, when the piston is drawn higher, than the circular opening *a*. The tube C is for the attachment of a smaller one, to be inserted into the nose, larynx, or trachea (*b*).

If the lungs are to be inflated, the extremity of the small tube must be inserted into one of the aërial passages, and the others properly shut. The piston being drawn up, and the orifice *d* closed with the finger, we press down the piston, and force the contents of the cylinder into the lungs.

(*a*) I think I have seen this opinion verified sometimes in young animals, where the lungs contained a considerable quantity of froth after submersion: if I inflated them fully in this state, the colour of the blood in the sinus venosus and left auricle was not apparently changed, and the heart did not renew its contractions, although it still retained the faculty of contraction. Some similar cases are mentioned, by the Dutch writers, in the human subject after submersion. The persons inspired several times of themselves, but still they did not recover; and this failure may perhaps be accounted for in the same manner.

(*b*) I am favoured with this instrument by Dr. Nooth, a gentleman distinguished as much for liberality as genius, to whom the arts are indebted for several valuable inventions, which are commonly attributed to others.

A few seconds after, we draw up the piston, and the air passes from the lungs into the cylinder again. Then we remove the finger from the orifice *a*, press down the piston, and the greater part of this expired air escapes into the atmosphere. After this, we draw up the piston a second time, while the orifice *a* continues open, and a volume of fresh air enters into the cylinder, which may be forced into the lungs in the same manner.

But where it is necessary to extract water from the lungs before the inflation, we begin the process with the piston down; and when the small tube is inserted, as before, we draw up the piston, until the lower part E is contiguous to the orifice *a:* the water will then rise up from the lungs into the fauces, or cylinder. If the water rise into the cylinder, it may be evacuated by detaching the tube C from its connexion with the smaller tube: and this may be repeated once or twice; but always with great caution, lest the pulmonary vessels be injured. Then we proceed to inflate the lungs in the manner just described (*a*).

If the inflations be continued carefully and slowly for several minutes in this manner, the contractions of the heart

(*a*) Other machines have been lately proposed to inflate the lungs in this disease, and particularly one by Mr. Kite of Gravesend, and another by Mr. Hurlock of St. Paul's Church-yard; two surgeons of considerable eminence and respectability. I have not yet employed these machines on animals, but am informed, that they are extremely well calculated for the purpose. It appears, however, that they are encumbered with valves; and in the choice of an instrument for a business of this kind, which is sometimes trusted to the awkward and ignorant part of mankind, I should be disposed to prefer the most simple, where there are neither valves, nor stop-cocks, and where the quantity of air to be injected is clearly measured: but the value of these different instruments must be determined by future experience.

will be gradually renewed, and the other functions will soon return, without any other inconvenience than a difficult and stertorous respiration, which often continues a short time afterward; this inconvenience arises from some water still remaing in the lungs, which will be gradually evaporated by the expired air.

In situations where dephlogisticated air can be procured to inflate the lungs, it should be always preferred to atmospheric air. I have employed it in several instances in the smaller animals, and the recovery was commonly more expeditious than where atmospheric air was used; but I have not yet been able to recover an animal with dephlogisticated air, where good atmospheric air had been ineffectual.

Several other remedies have been recommended for this disease by different writers, and many of them are said to have been employed with success; but as the circumstances of their application, and the progressive changes in the body, are not minutely detailed, nothing can yet be said to be determined in favour of their particular efficacy. If indeed we were to judge of them from the history of their introduction, or from their ordinary operation on living bodies, little assistance would be expected from them.

Some of them were suggested from a false notion of the nature of the disease (*a*); others, from a mistaken opinion

(*a*) Bleeding, friction, and succussion. See Mémoires sur les Noyés, &c. par Mons. *Louis*, *Nederlandische Jaarboeken*, April 1758. *Dissertatio de Syncope*, *Auctore Hieron. Queye*.

I

of the principal seat of life (*a*); and some, from an ignorance of the most effectual means to solicit the principle of life into action (*b*): and none of them appear fitted to excite the natural contractions of the heart by direct agency: but since they are now introduced into practice on the authority of several learned societies, experiment alone ought to decide concerning their efficacy.

In the cases recorded of their successful application the bodies were warmed: now it has been shown in the sixth section, that the presence of heat alone is often sufficient to produce a recovery; and therefore, whilst the remedies are applied under these circumstances, the success cannot with propriety be attributed to their operation: and if they be applied when the temperature of the body is so low, that warmth cannot be supposed to contribute to the recovery, it is known from experiment, that no sensible change will be produced: the concurrence of heat is therefore necessary to their success.

Still, however, it may be supposed, that the concurrence of respiration is not necessary.

But this may be determined by applying these remedies in conjunction with heat, by attending to the progressive

(*a*) The application of different substances to the skin, the stomach, the intestines, the parts of generation, the nose, the fauces, the extremities of the fingers, &c. *Dissertatio de Causâ Mortis Submersorum, Auct. Jacob Gummer.*

(*b*) The employment of electricity, alcohol, volatile alkali, tobacco, essential oils, and all the acrid and stimulating substances.

Tract. Isnard. pag. 13. Obs. 5.

Ranchinus in Tract. de Morb. subit. Cap. 12.

Ælius in Tetrab. Cap. 49.

Le Voyage d'Acadie, pag. 190.

changes in the body during their successful application, and by observing if the other functions return before the respiration be renewed.

I applied these remedies singly to several animals under these circumstances, and attended carefully to the progressive changes in the body: some of them were recovered, and others were not; and in all the successful cases the progression of the recovery was as follows. The organs of respiration began first to move, two or three inspirations followed; the contractions of the heart were gradually renewed, and then the other functions followed.

In these cases it appears, that these remedies were not often attended with success; and even when they were so, the other functions did not return until respiration began: the concurrence of respiration is therefore necessary to their successs; and if it could be granted, that they entirely, or even in part, occasioned the recovery in these cases, it must evidently be by operating on the organs of respiration, by exciting them to action, by producing an inspiration, and applying to the heart its natural stimulus; thus inducing it to contract, and to restore the suspended functions.

Hence, if we allow to these remedies all the efficacy that partiality can claim, it can only be said, that they sometimes produce by indirect means, what may be always done directly by the method we have recommended: surely then we ought not to hesitate in preferring a remedy which can be applied directly, and which operates with certainty and expedition, to one that is slow, indirect, and uncertain.

In situations, indeed, where the means of artificial inflation cannot be immediately procured, these remedies may be employed for the moment; but every person ought to be apprised of their insufficiency, and of the necessity of employing artificial inflation, even after their application has failed.

Haller mentions a case, where the ordinary remedies had been employed an hour without any sensible effect, and yet by inflating the lungs the patient was quickly recovered (*a*); and I have often seen the smaller animals recovered by inflation, after the other remedies had failed.

Notwithstanding these facts and conclusions, some practitioners may be still disposed to think with the ancients. With such, authority will be more powerful than argument; yet, since those who will not be shaken by the labour of youth, may sometimes yield to the judgment of age, we shall venture to address them in the language of the candid Sydenham.

" Atque hoc mihi, suffragante experientiâ multiplici,
" compertissimum est: experientiâ, inquam, optimâ duce
" et magistrâ, ad cujus leges et normam nisi exerceatur
" Medicina, eam prorsus exulare satius esset: luditur
" enim (quod aiunt), de corio humano plus satis, cum
" hinc empirici, neque morborum historiam, nec metho-
" dum medendi callentes, et receptis tantum freti; ist-
" hinc sciolorum vanissimi spem omnem in affectato artis
" ambitu, et speculationibus utrinque pari fere momento

(*a*) Disputat. ad Morb. curand. et Hist. pertinent. tom. vi. pag. 318.

" disceptatis ponentes. Ea demum Praxis, eaque sola " ægris mortalibus opem feret quæ indicationes curativas " ex ipsis morborum phænomenis elicit, dein firmat ex- " perientiâ, quibus gradibus magnus Hippocrates ad cœ- " lum ascendit."

FINIS.